Reviewer’s report

Title: Effects of Attentional Bias Modification on Residual Symptoms in depression. A Randomized Controlled Trial.

Version: 2 Date: 25 Mar 2019

Reviewer: Reviewer 2

Reviewer’s report:

PEER REVIEWER ASSESSMENTS:

OBJECTIVE - Full research articles: is there a clear objective that addresses one or several testable research questions? (Brief or other article types: is there a clear objective?)

Yes - there is a clear objective

DESIGN - Is the current approach (including controls and analysis protocols) appropriate for the objective?

Yes - the approach is appropriate

EXECUTION - Are the experiments and analyses performed with sufficient technical rigor to allow confidence in the results?

No - there are minor issues

STATISTICS - Is the use of statistics in the manuscript appropriate?

No - there are issues with the statistics in the study

INTERPRETATION - Is the current interpretation/discussion of the results reasonable and not overstated?

No - there are minor issues

OVERALL MANUSCRIPT POTENTIAL - Has the author addressed your concerns sufficiently for you to now recommend the work as a technically sound contribution? If not, can further revisions be made to make the work technically sound?
Probably - with minor revisions

PEER REVIEWER COMMENTS:

GENERAL COMMENTS: I appreciate the authors' effort in this revision, the statistics are much clearer to me now.

1) For the post hoc tests, the authors state that "There are no post-hoc t-tests in results, as time is the pre-post factor that interacts with the dichotomous ABM vs. placebo variable. There is therefore also no variable that distinguish e.g. symptom improvement from symptom worsening or combined factors that warrants post-hoc comparisons."

This may be true for Figure 1 (Line 258-265), although one can still do post-hoc comparison between groups at same time (e.g., placebo vs. ABM at baseline, or at 2 week follow up) or within group but across time (e.g., placebo baseline vs. placebo 2 week) to figure out what exactly is the driving force behind the interaction. To say that there's a significant interaction, and then stop the analysis, is somewhat surprising to me, because we need to know what's driving the interaction.

This is no longer an issue in the analysis from Line 267 to 276, because the scores between ABM and placebo are matched at baseline, so post-hoc tests should be doable and without any difficulty to explain the observed significant effect (if any).

I think it's okay to not have any significant post hoc tests, it only means that we have to be more conservative when explain the origin (or direction) of the significant interaction. Right now the manuscript emphasizes the therapeutic potential of ABM, but that's not entirely clear yet (see my next comment).

2) Since the effect of ABM seems to be driven by the worsening of the placebo group (whereas the ABM group remains stationary), there are 2 possibilities for the ABM effect: 1) patients' HRSD worsens over time, but ABM is improving something else, so the two cancels each other out, or 2) ABM is targeting the exact thing that's worsening in placebo group, and prevents it from worsening, so ABM is protective against worsening of depression. Although the 2 sound similar, #1 is indicating an improvement in one domain, but worsening in another separate domain (the improvement account); whereas #2 is claiming no improvement, but no worsening either (the protective account). Might be worth mention in the discussion, and based on Figure 3, probably #1 the improvement account is more likely.

3) But this brings me to my previous review, and I still stand by my last comment from my last review: "Since Figure 3 clearly shows that ABM works differently for everyone, and may even backfire for some patients, the key finding from this study for me is that "AB change may be a useful clinical measure since it is sensitive to individual differences in HRSD", as opposed to ABM as a treatment."
Even if we think that ABM is inducing improvement (instead of protective), it clearly does not work for everyone even in the ABM group. There are quite a few individuals who had worse HRSD after ABM, which is okay, because no treatment is perfect. But the most intriguing finding is that these people's worsening in HRSD is actually (mildly) predicted by their ABM performance. This, to me, is actually the most valuable finding from this study because this can actually predict treatment outcome and the variances within the patients (not a lot of measures can do that). This is why I'm perplexed by the direction of this manuscript, it's trying to describe ABM as a potentially useful treatment, which is only half true because it's clearly not for everyone (and can even make some people worse; though I think the authors can argue that worsening is on par with the placebo group so that ABM is simply not working for these people, but the worsening is not coming from ABM). But the ability of AB change to predict HRSD change is interesting (because we can explain variability of treatment outcome using an attentional and mechanistic account), yet the manuscript downplays this point. But anyway, I leave it to the authors to decide whether this point is worth mentioning in the manuscript or not.

REQUESTED REVISIONS:

Address my comment #1 and #2 from above.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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